CORRELATION, CAUSATION AND WRIGHT'S THEORY OF "PATH COEFFICIENTS"[1]

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INTRODUCTION

What occasions a result? What is its determining cause?

We have an answer to questions of this sort in many specific cases, but none of the attempts to produce a general formula universally applicable for the solution of such questions has been entirely satisfactory. The present paper is a critical discussion of the latest solution offered, the method of "path coefficients" as proposed by WRIGHT (1921 a).

The conscious attempts to obtain a mathematical measure of causation, or to establish a mathematical criterion by which to test the truth of the statement that one event is the cause of another, have been, in the main, recent developments, but they are all essentially refinements of the simple method of concluding, because the observer has never known one event to happen without being followed by the other, that the two are therefore cause and effect. Although there may possibly be a few cases that appear to be exceptions when the observer is forming his conclusions, he is apt to reject them as being due to certain factors which he overlooked. This whole procedure is simply a non-mathematical way of determining roughly the degree of association or of correlation between the two events and regarding a high correlation either as causation itself or as evidence of a

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"causal relation." GALTON (1889) says in regard to correlation between organs:

> "It is easy to see that correlation must be the consequence of the variations of the two organs being partly due to common causes. If they were wholly due to common causes, the co-relation would be nil. Between these two extremes are an endless number of intermediate cases."

This is the opinion of the man who appears to have been the first to conceive the idea of mathematical correlation (PEARSON 1920). The works of BRAVAIS and of GAUSS are treatments of the probability of errors of observation, and afford no basis for a claim that either of them discovered correlation.

"Causation" has been popularly used to express the condition of association, when applied to natural phenomena. There is no philosophical basis for giving it a wider meaning than partial or absolute association. In no case has it been proved that there is an inherent necessity in the laws of nature. Causation is correlation.

In his "Grammar of Science," PEARSON (1900), who developed the product-sum correlation coefficient now used, says in regard to scientific law and causation:

> "Law in the scientific sense only describes in mental shorthand the sequences of our perceptions. It does not explain *why* those perceptions have a certain order, nor *why* that order repeats itself; the law discovered by science introduces no element of necessity into the consequences of our sense impressions; it merely gives a concise statement of *how* changes are taking place. That a certain sequence has occurred and recurred in the past is a matter of experience to which we give expression in the concept *causation*; that it will continue to occur in the future is a matter of belief to which we give expression in *probability*. Science in no case can demonstrate any inherent necessity in a sequence, nor prove with absolute certainty that it must be repeated."

. . . . . .

> "When we say that we have reached a 'mechanical explanation' of any phenomenon, we only mean that we have described in the concise language of mechanics a certain routine of perceptions. We are neither able to explain why sense-impressions have a definite sequence, nor to assert that there is really an element of necessity in the phenomenon. Regarded from this standpoint, the laws of mechanics are seen to be essentially an intellectual product, and it appears absolutely unreasonable to contrast the mechanical with the intellectual when once these words are grasped in their accurate scientific sense."

. . . . . .

> "No phenomenon or stage in a sequence has only one cause, all antecedent stages are successive causes, and, as science has no reason to infer a first cause, the succession of causes can be carried back to the limit of existing knowledge and beyond that *ad infinitum* in the field of conceivable knowledge. When we scientifically state causes we are really describing the successive

stages of a routine of experience. 'Causation' says JOHN STUART MILL 'is uniform antecedence' and this definition is perfectly in accord with the scientific concept."

. . . . . .

"The causes of any *individual* thing thus widen out into the unmanageable history of the universe. The causes of any finite portions of the universe lead us irresistibly to the history of the universe as a whole."

The above quotations are made, not as an appeal to authority, but because Professor PEARSON has already inimitably summarized the subject.

The theory of planetary motion is an intellectual concept that has been built up to describe, at least approximately, the observed events of the movements of the planets. If the theory holds, certain things *must* of logical necessity be true, but we must beware lest we unconsciously and illogically think that the necessity that lies in the concept also inheres in the order of nature which the concept attempts to describe.

The idea of determination, in the sense of causes fixing beforehand the nature of the effect, is based upon the belief in an inherent necessity in the order of things. We have seen that no such necessity can be proved. Therefore, determination should be used only in the sense of an ability to predict with fair accuracy the value of an effect when the values of its principal causes are known. This ability is based upon our knowledge of the degree of association between the causes and the effect.

To contrast "causation" and "correlation" is unwarranted because causation is simply perfect correlation. Incomplete correlation denotes partial causation, the effect here being brought about by more than one important cause. Many things show either high or perfect correlation that, on common-sense grounds, can not possibly be cause and effect. But we can not tell *a priori* what things are cause and effect and a conflict between our observations and our "common-sense" belief may be due either to an unwarranted belief or else to the calculation of our coefficients of correlation from too few data.

If Röntgen rays be directed against a brick wall, one can see through it. But it would indeed be difficult to imagine two more dissimilar things than Röntgen rays and sight through a brick wall; and yet, because these are invariably correlated, they are now so accepted. An example of high correlation and no causal relation might be the correlation over a two year period between the weight of a child born in 1917 and the tonnage production of ships in the United States. Here the data are evidently insufficient. We know that children born in 1917 grew at practically the same rate as children have always grown, but that ships were produced at a much faster rate in order to meet the war needs. Therefore if we correlate the weight of children for their first two years of life with the tonnage production of ships over any long period we may be sure that the correlation coefficient would be practically zero.

It seems clear that perfect correlation, *when based upon sufficient experience*, is causation in the scientific sense.

THE METHOD OF PATH COEFFICIENTS

This method is claimed by WRIGHT (1921 a, b) to provide a measure of the influence of each cause upon the effect. Not only does it enable one to determine the effects of different systems of breeding, but provides a solution to the important problem of the relative influence of heredity and environment. To find flaws in a method that would be of such great value to science if only it were valid is certainly disappointing. The basic fallacy of the method appears to be the assumption that it is possible to set up *a priori* a comparatively simple graphic system which will truly represent the lines of action of several variables upon each other, and upon a common result.

In his introduction WRIGHT (1921 a) states:

> "The method depends on the combination of knowledge of the degrees of correlation among variables in a system with such knowledge as may be possessed of the causal relations. In cases in which the causal relations are uncertain the method can be used to find the logical consequences of any particular hypothesis in regard to them."

We have to set up a graphic system of the way we think the variables act upon each other. If we have enough observed correlation coefficients, we calculate the path coefficients and the coefficients of determination. The results are then compared with what we expect or have observed to be true in nature, and if they are in pretty close agreement our hypothesis is accepted, and we are to regard the system as showing the true relations between variables. If they are not in agreement, the hypothesis upon which we built our system must be wrong and a new one will have to be tried. But even if the observed and calculated values of the correlation coefficients agree, we can by no means be sure that we have set up the true system. An infinitude of values of x and y satisfy the simple equation $x^2+y^2=1$. The arrangement of the system depends entirely upon the judgment of the observer, and no *test* of that judgment follows in the least.

In all set-ups, or diagrams of systems, it is necessary to cut off the lines of causation at points not very far back in the chain of causes. This leaves two or more cause groups with nothing behind them, although it is inconceivable that these groups have not some common causes. If we put into our system all important causes we know of, and all the important causes of these, and so on back, we would cover the whole universe and even then find no logical stopping place. There is absolutely nothing in the method to tell us how far back we should go. Apparently WRIGHT himself goes back as far as the observed correlation coefficients, which are needed to solve the equations, will permit. But extension backwards will change the values of path coefficients and coefficients of determination, and may also render the whole system unsolvable.

Two methods for the solution of the hypothetical systems are given; the direct, using determinants; and the indirect, using simultaneous equations. The indirect method is said to be less laborious than the direct, and this method "is more flexible in that it can be used to test out the consequences of any assumed relation among factors." (WRIGHT 1921 a, p. 578). Also (loc. cit., page 579):

"One should not attempt to apply in general a causal interpretation to solutions by the direct methods. In these cases, determination can usually be used only in the sense in which it can be said that knowledge of the effect determines the probable value of the cause. This is the sense in which PEARSON's formula for multiple regression must be interpreted."

Measures of association or correlation are provided by mathematics, but we have no mathematical test which will enable us to tell absolutely whether or not to interpret any particular case as one of causation. We can not be sure that we have taken enough cases or a sufficiently large number of variables into consideration. By using the correlation coefficient we *know* that, in the sample of the universe which we have studied, certain variables were more or less closely associated than others, as indicated by the value of r, the coefficient of correlation, provided that the variations of each variable followed the Gaussian, or normal, curve of error. From this knowledge we are led to *believe*, either that the sample tried is not a fair one and another one is needed, or that certain causal relations probably do or do not exist. Statistical methods, particularly multiple correlation, indicate causes when they are used with common sense and upon the data of critical experiments. But the method of path coefficients does not aid us because of the following three fallacies that appear to vitiate this theory. These are (1) the assumption that a correct system of the action of the variables upon each other can be set up from *a priori* knowledge; (2) the idea that causation implies an inherently necessary connection between things, or that in some other way it differs from correlation; (3) the necessity of breaking off the chain of causes at some comparatively near finite point. The applications of this theory in the latter part of this paper give impossible results and illustrate faults in the method.

THE MATHEMATICS OF PATH COEFFICIENTS

The section on Definitions in WRIGHT'S paper opens with the following sentences (italics mine):

"We will start with the assumption that the direct influence along a given path can be measured by the standard deviation remaining in the effect after all other possible paths of influence are eliminated, *while variation of the causes back of the given path is kept as great as ever, regardless of their relations to the other variables which have been made constant.* Let X be the dependent variable or effect and A the independent variable or cause. The expression $\sigma_{X.A}$ will be used for the standard deviation of X, which is found under the foregoing conditions, and may be read as the standard deviation of X due to A."

If X is regarded as being completely determined by A and B, WRIGHT'S $\sigma_{X.A}$ is somewhat like YULE'S $\sigma_{X.B}$, the standard deviation of X when B is held constant; and when X is completely determined by A, B and C, it is somewhat like YULE'S $\sigma_{X.BC}$. The physical interpretation of $\sigma_{X.B}$ and $\sigma_{X.BC}$ is very easy. If from a large group, all the cases having the same-size B's, or the same-size B's and C's, were picked and the standard deviation of the X's in this new group was found, the result would be the familiar $\sigma_{X.B}$ or $\sigma_{X.BC}$. But to make this correspond to WRIGHT'S $\sigma_{X.A}$ we should in some manner have to keep all the variables back of B, which affected A, *just as variable as before.* How this might be done is difficult for the mind to grasp.

In figure 1, if we wish to get WRIGHT'S $\sigma_{X.A}$ we must not let any action come along the path BX but must make as much as before come along $A\ X$, $A\ D$ and $A\ C$. If A and B are correlated, and if B is given a constant value it follows that the variation in C must be reduced and the path coefficient along the line $A\ C$ changed. In holding constant a variable we are really picking out only observations of it that have the same value and are considering the causes and effects of this new group. The results of a constant effect are obviously less variable than those of a variable effect; and, although there may conceivably be some compensatory changes in the causes, it seems impossible that they should vary as much in producing a constant effect as in producing a variable one. This simple point of logic WRIGHT appears to have overlooked.

In a paper in Genetics, WRIGHT (1921 b) says:

"A path coefficient differs from a coefficient of correlation in having direction. The correlation between two variables can be shown to equal the sum of the products of the chains of path coefficients along all the paths by which the variables are connected."

The pure mathematics by which this is shown is apparently faultless in the sense of mere algebraic manipulation, but it is based upon assumptions which are wholly without warrant from the standpoint of concrete, phenomenal actuality.

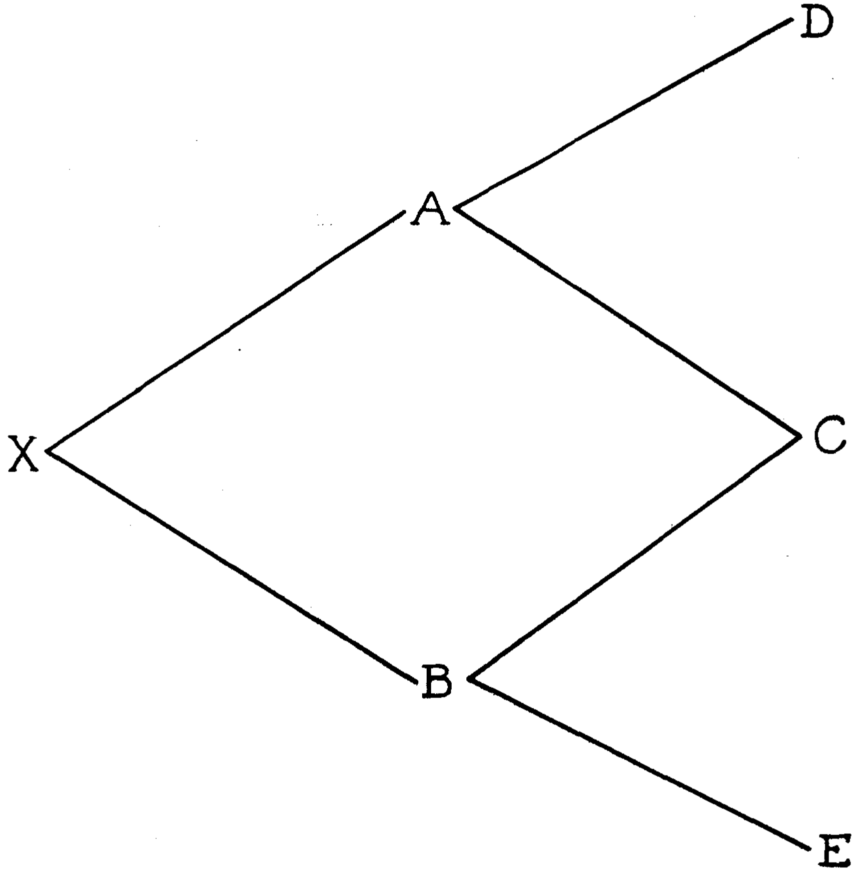

FIGURE 1.—An effect, X, determined by two correlated causes.

WRIGHT'S GUINEA-PIG EXAMPLE

The guinea-pig is "intended merely to furnish a simple illustration of the method" (WRIGHT 1921 a, p. 570). It shows us how to measure the relative importance in determining the birth weight of guinea-pigs, X, of the factors Q, prenatal growth curve; P, gestation period; L, size of litter; A, heredity and environmental factors which determine Q apart from size of litter; C, factors determining gestation period apart from size of litter. The "prenatal growth curve" is apparently the average growth per unit of time during the gestation period. If we multiply the average growth by the time we necessarily get the birth weight, but it is impossible to get the average growth until the total growth and the time are known. If $\frac{X}{P}=Q$, then any two of the variables mathematically determine the third. It is as logical to say that the birth weight and the gestation period determine the prenatal growth curve, as to say that the gestation period and the prenatal growth curve determine the birth weight.

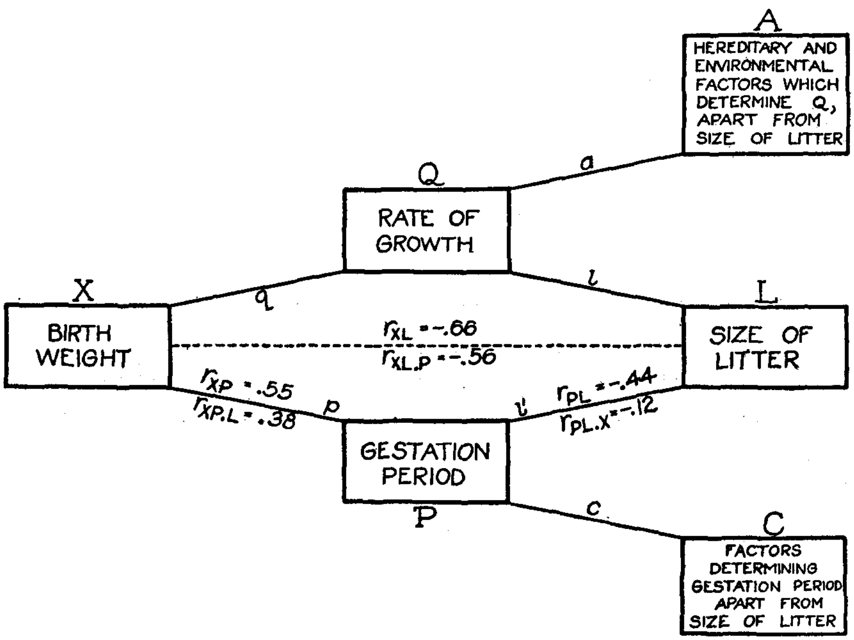

FIGURE 2.—The system set up by WRIGHT (1921 a) for finding the relative importance of factors determining the birth weight of guinea-pigs.

In solving the guinea-pig problem three things are known from experience (figure 2). These are the correlations between birth weight and interval between litters, which is assumed to be the gestation period if less than 75 days, $r_{XP}=+.5547$; birth weight and size of litter, $r_{XL}=-.6578$; and between gestation period and size of litter, $r_{PL}=.4444$. These are the realities. From the general equation where the coefficient of correlation is the sum of the products of the path coefficients along all the chains of causes connecting the two variables, he derives three equations:

(1) $r_{XP} = p + q\,l\,l'$
(2) $r_{XL} = q\,l + p\,l'$
(3) $r_{PL} = l'$

We might get equations (1) and (2) directly from figure 2, but to be consistent throughout we must get equation (1) from the system shown in

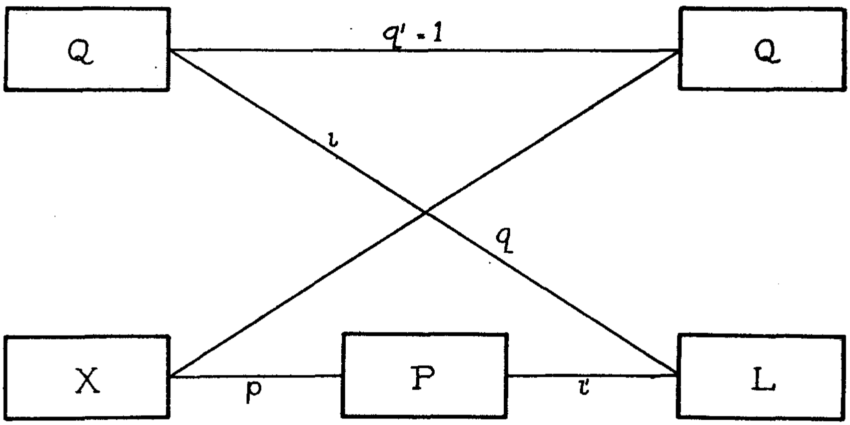

FIGURE 3.—System from which equation (1) would be obtained.

figure 3, equation (2) from figure 4, and equation (3) from figure 5. In each of these systems one of the variables is made a cause of itself with a path coefficient of unity between variable as cause and variable as effect.

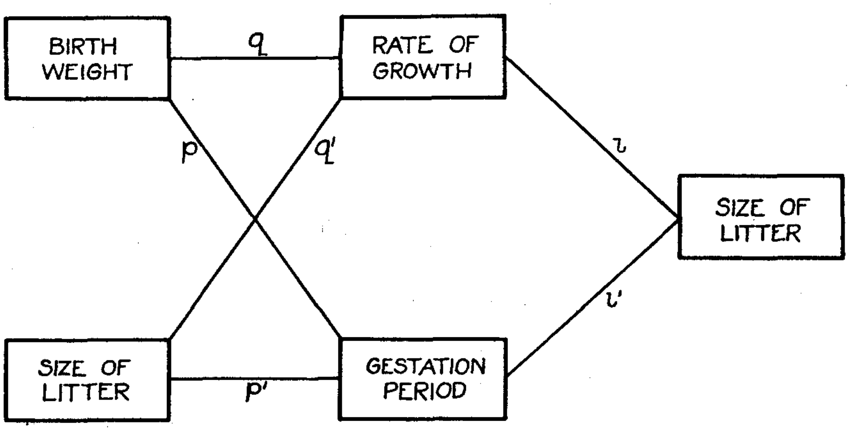

FIGURE 4.—System from which equation (2) would be obtained. $q'l = 1$, and $p'l' = 1$.

This seems rather forced, but if we attempt to obtain the relation from the original diagram, what is there to prevent our setting $r_{PL} = l' + pq\,l$; that is, following all the possible paths of the original set-up in getting equation (3)? Perhaps in tracing the chains of causes we are not allowed to come to the effect through something that follows it. Such a rule would be meaningless when the true relation of cause and effect, that of invariable association, is kept in mind.

Three more equations are based upon the fact that the sum of the coefficients of determination of any effect must be equal to unity. These coefficients are simply the squares of the path coefficients between cause

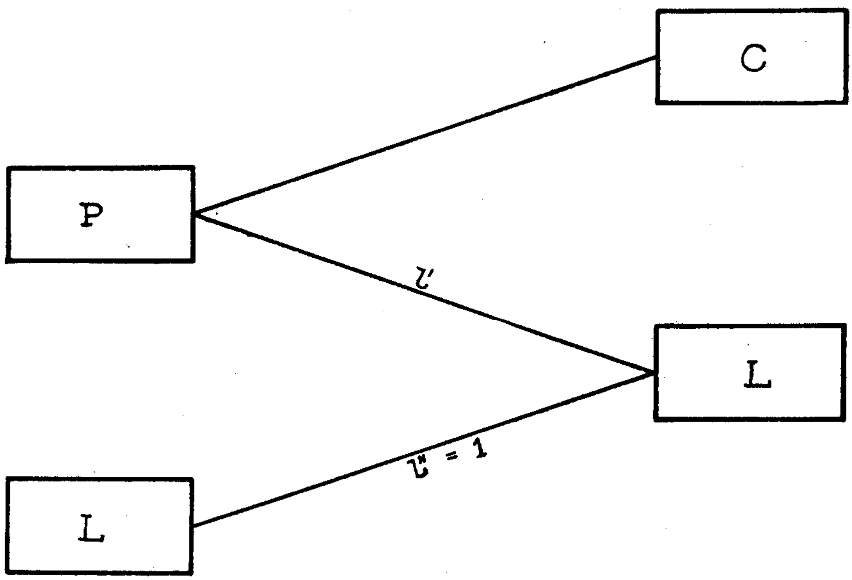

FIGURE 5.—System from which equation (3) would be obtained.

and effect; except when there are correlated causes, when there is also a coefficient of determination which represents the action of the correlated causes taken together. This coefficient is twice the product of the path coefficients from each cause to the effect, times the coefficient of correlation between the causes. The additional equations in this case are

$$(4)\ q^2+p^2+2q\,p\,l\,l'=1$$
$$(5)\ a^2+l^2=1$$
$$(6)\ l'^2+c^2=1$$

The values obtained from the six equations are assumed to be measures of realities if the diagrams accurately represent the causal relations. Figure 6 shows the values obtained for each path coefficient. No value of the probable error of any constant is given by WRIGHT. If the method of path coefficients were valid, a knowledge of the probable error of any constant would be essential in many cases. The correlation between size of litter and gestation period for constant birth weight, using only the observed r's the writer computed and found to be $r_{PL.X} = -.12$. How are we to account for the difference between this value and $r_{PL} = l' = -.44$? The $r_{PL.X}$ means that when we select groups of guinea-pigs of the same birth weight the correlation between size of litter and gestation period is greatly reduced, and the path coefficient and coefficient of determination are correspondingly reduced, the latter taking the value $-.014$. Therefore,

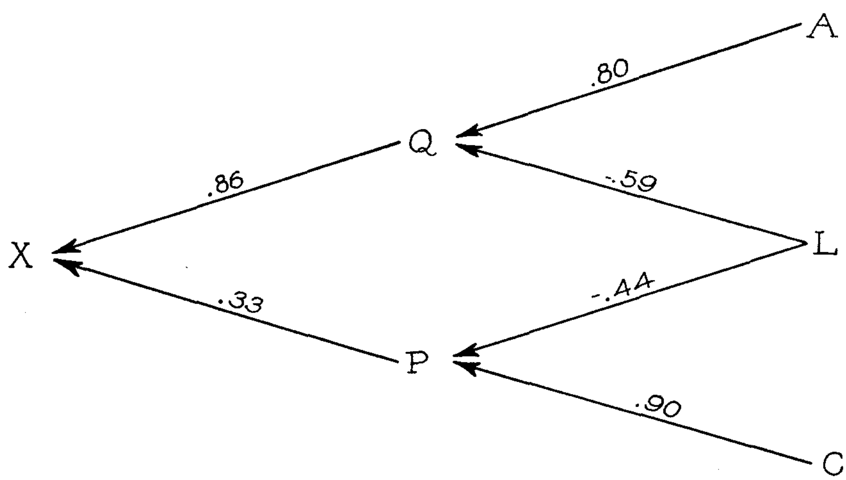

FIGURE 6.—Showing the values obtained by WRIGHT (1921 a) for the path coefficients between the factors determining birth weight of guinea-pigs. See figure 2.

when guinea-pigs of equal birth weight are considered, the size of the litter has practically no effect upon the gestation period.

TEST OF WRIGHT'S METHOD

Except in unusual cases we can check the results of this method of WRIGHT's only by testing them with what we *think* on common sense grounds ought to be true. In the hands of a man well acquainted with the realities in the field he is investigating, this method would be likely to lead to results not far from the truth, because if any values appear to be inconsistent, a new set-up of causes and effects will be made. Guesses by a trained man would be on the whole quite as good and much less work; whereas an untrained man can not be sure of the validity of his results at all, because he is not familiar with the realities in the field of study.

Let us attempt to apply this method to two examples where we know more of the correlation coefficients, and can get our path coefficients and coefficients of determination in more than one way.

Example 1

We are interested here in the relative part played by the number of seeds per pod, and the number of ovules per pod, in determining the seed weight of the seeds produced. We set up the diagram shown in figure 7, making (1) seed weight, be determined by (2) seeds per pod, (3) ovules per pod, and (4) other causes than (2) and (3). Seeds per pod is determined by (5) pods per plant and (6) other causes than (5) which affect (2). Ovules per pod is also determined by (5) and by (7) a group of other causes. Behind these we do not go as we have not enough observations

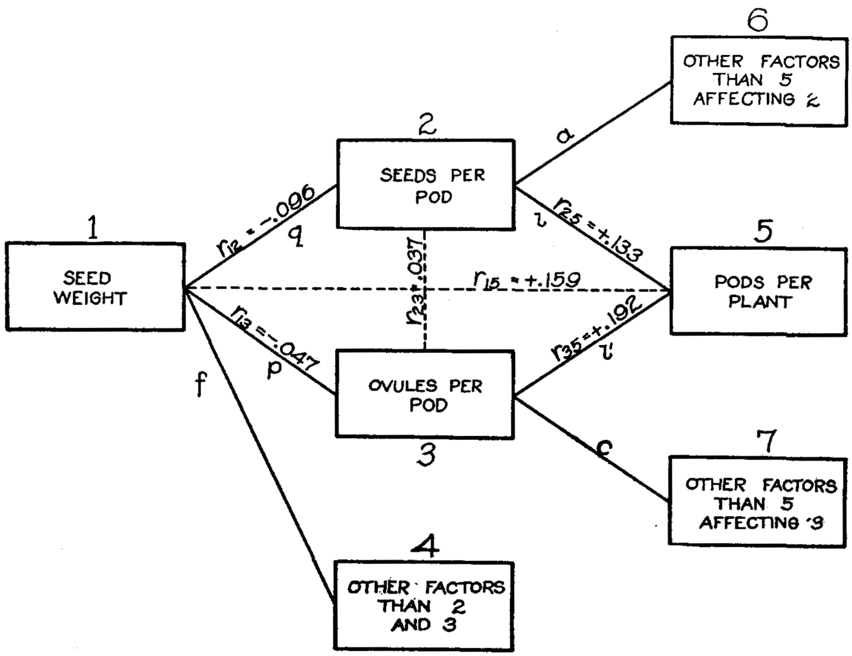

FIGURE 7.—The system set up for example 1, showing the observed correlation coefficients.

to solve a more complicated system and we will assume that we are to be satisfied with approximate results. This figure is identical with the diagram (figure 2) used by WRIGHT (1921 a) in setting up his equations, except that we have an all-other-causes path affecting (1) directly. This enters only in the equation which makes the summation of the coefficients of determination equal to unity. The correlation coefficients shown in the figure are from J. ARTHUR HARRIS (1913 a, 1913 b, 1916) and although of low absolute values, they are very probably significant, because the probable errors where given are extremely small, and all the constants appear to have been based upon a very wide experience.

The equations involving only the first powers of the path coefficients are:

$$(1)\ r_{13}=p+q\,l\,l'$$
$$(1\text{ bis})\ r_{12}=q+p\,l\,l'$$
$$(2)\ r_{15}=q\,l+p\,l'$$
$$(3)\ r_{25}=l$$
$$(4)\ r_{35}=l'$$

Taking r_{13}, r_{15}, r_{25}, and r_{35} as known, we will use the above equations to get the path coefficients and the coefficients of determination.

Substituting the numerical values of r_{13}, l and l' in equation (1) we have

$$p=-.047-(.133\times.192)\,q$$
$$=-.047-.025536\,q.$$

Equation (2) now becomes

$$.159=.133\,q+.192\,(-.047-.025536\,q)$$
$$q=1.3117.$$

Whence

$$p=-.08049$$

These values give in (1bis)

$$r_{12}=1.312-(.080\times.133\times.192)$$
$$=+1.310$$

Assuming r_{12}, r_{15}, r_{25}, and r_{35} known and solving for r_{13} we get

$$q=-.119$$
$$p=+.911$$

These values in (1) give

$$r_{13}=+.908$$

As a correlation coefficient can never be greater than 1, $r=1.310$ is impossible. The computed values of r_{12} and of r_{13} are in both cases more than twelve times the observed values and opposite to them in sign. Such results are ridiculous.

Let us now test this system for the coefficient of determination of the causes which we considered as "all other causes," group 7. From the principle that the sum of the coefficients of determination must be unity, we have

$$(5)\ q^2+p^2+2\,qpll'+f^2=1$$

Substituting in this the values obtained for p and q in our first solution, we have

$$1.7213+.0064-.0054+f^2=1$$
$$f^2=-.7223$$

Substituting the values from the second solution, we have

$$f^2=.1615$$

What does this really tell us about the effects of the factors not included? In the first case the determination is negative and has no meaning. The proportion of the standard deviation of the seed weight due to all other causes than those acting through seeds per pod and ovules per pod is $\sqrt{-.7223}=f$ according to WRIGHT's theory. This is an imaginary standard deviation, a thing not encountered in statistics. In the second case we find that the unknown causes play apparently a real although small part in determining the seed weight. But there is no inherent or logical reason *in* WRIGHT's *theory* why the first solution is not as good as the second.

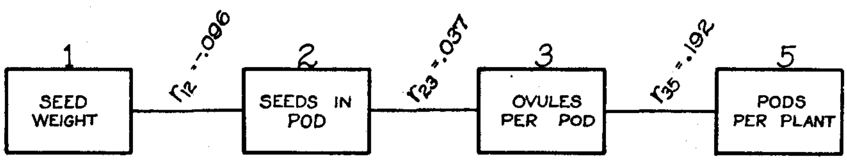

FIGURE 8.—An alternative set-up for the seed-weight example, giving observed correlation coefficients.

WRIGHT gives a special formula for finding the coefficient of determination of factors not specifically included in a system when correlations between the factors included are known. In our case the appropriate formula seems to be the one for two known correlated causes acting upon the effect. Using this formula and the observed r's gives the coefficient of determination between not-included causes and the effect, or f^2, equal to 0.9944, but using the calculated r's gives f equal $\sqrt{-1.482}$. Here is another case of two widely different values for the same constant, and one value is again imaginary and impossible.

It may be contended that the causal connections are really a straight line from pods per plant to ovules per pod, to seeds per pod, to seed weight. In this case we draw our diagram as in figure 8. If we multiply together the path coefficients from 1 to 5, we should obtain the correlation between 1 and 5 because there are no common causes and the path coefficients therefore are equal, on WRIGHT's theory, to the coefficients of the correlation. By multiplying we find $r_{15}=-.000682$. This is not even one percent of the observed value. Evidently the theory works with this setup no better than with the last.

Example 2

This is an application of the method of path coefficients in an attempt to determine approximately the relative importance of some factors influencing the amount of heat produced in human basal metabolism. As shown in figure 9, we assume that stature determines in part body weight and body surface, and that these, with a group of other factors, determine the heat produced. The correlation coefficients given in the figure are taken directly from HARRIS and BENEDICT (1919) with the exception of the one between surface and stature which had to be computed from the raw data given in the reference. This figure is identical in form with figure 7

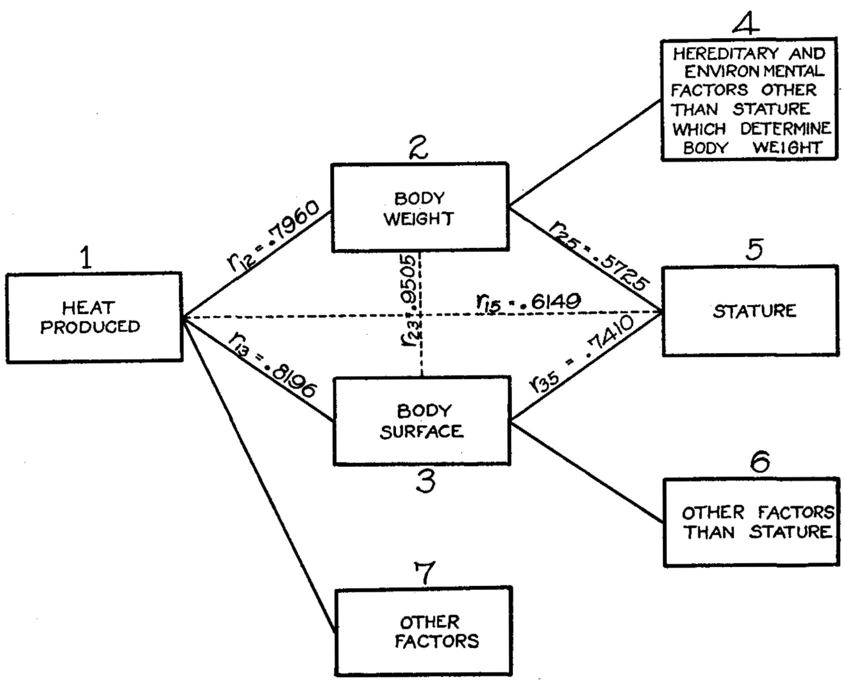

FIGURE 9.—The system set up for example 2, showing the observed correlation coefficients.

used in example 1, and the same set of equations is therefore applicable. Without repeating these equations we will give the results obtained by solving them. Treating r_{12} as unknown we find its value to be $+.3718$, or less than one-half of the observed value. Treating r_{13} as unknown we find its value to be $+.5997$ or about three-fourths of the observed value. The correlation between the heat produced, and factors other than body weight and surface and those acting through them, is found to be either (a) $r_{10}=+.5703$, or (b) $\sqrt{-1.3893}$, or (c) $+.338$, depending upon the values used for r_{12}, and r_{13}. The values of the path coefficients are found to be $p = +.8072$ or $+.3196$, and $q = +.0293$ or $+.6604$. The results of this example, like those of the preceding, are inconsistent with themselves and with reality. When we see that the path coefficients are unreliable in these cases where we can check them, we are not likely to place any great confidence in them where they cannot be checked.

Should the criticism be made that these examples give absurd results because the true action of the variables upon each other is not truly represented in the diagram the writer would reply, first, that such criticism but strengthens one of his main points; namely, that it is impossible to tell *a priori* how the system should be set up, and that the closeness of agreement between calculated or expected and observed values is an unscientific criterion by which to judge the validity of such a system; and second he would invite careful examination from a biological standpoint of his diagrams and WRIGHT's, with a view of the reader's seeing for himself whether the one set is more unfair or less related to the probable truth than the other.

CONCLUSION

We therefore conclude that philosophically the basis of the method of path coefficients is faulty, while practically the results of applying it where it can be checked prove it to be wholly unreliable.

The writer believes himself still open-minded on WRIGHT's proposition, but has an even more intense conviction that before that author's contribution to the theory of partial correlation can be taken seriously he will have to bring forward evidence altogether more cogent in respect of both logic and fact than any he has so far adduced.

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